

things needed besides technical knowledge to make a good engineer.

Mr. Gotshall's work is distinctly the more ambitious of the two, in that it seeks rather to point out the general principles applicable to all cases of electric railway projection, whereas Mr. Gonzenbach confines himself to the consideration in outline of a particular case. The student will derive from Mr. Gotshall's book a good idea of the importance of every detail in the original scheme, and will see how greatly the operating costs and the dividends may be affected by careful design throughout. He will also be able to glean some useful hints on the methods of dealing with promoters, landowners, and so forth, with whom, if he is ever called upon to draw up a scheme for an electric railway, he is likely to have much to do. Many of the details and particulars in both books are naturally not applicable to this country, but this does not materially detract from their value. M. S.

The Pests and Blights of the Tea Plant. Second edition. By Sir G. Watt and H. H. Mann. Pp. xv+429. (Calcutta, 1903.)

THIS work first appeared in 1898 as a report of particular investigations on tours, but is now a large volume of more than 400 pages, with numerous illustrations. The amount of information collected is enormous, and one may understand that no tea-planter can dispense with the work, the more so since such subjects as hybridisation and the different races of tea seed, weeding, tilling and cultural operations generally, drainage and manuring of tea, pruning and plucking, &c., are fully dealt with, in addition to the enumeration and description of the multitude of insect and fungus enemies which the long suffering shrub harbours.

By means of conspicuous marginal notes the authors have undone most of the disadvantages inevitable from their general method of lumping together scraps of information derived from all kinds of sources, the relative value of which, moreover, is generally capable of being sifted because the references are given; in spite of this, however, and indispensable as the encyclopædic information is, we think much might be done in improving the style if the materials were better woven into a more narrative and continuous form. Why is it that the introductory sections on general physiology of plants—the fundamental study without which the sequel is useless—are so often badly done in such works as this? Does it mean that the great schools of science have even yet not impressed their learning on the officials entrusted with such important treatises, or is it that an older generation of workers not familiar with modern researches dominates the situation?

Highways and Byways in Sussex. By E. V. Lucas. With illustrations by Frederick L. Griggs. Pp. xx+416. (London: Macmillan and Co., Ltd., 1904.) Price 6s.

MR. LUCAS himself aptly describes his book. He tells the reader:—"My aim has been to gather a Sussex bouquet rather than to present the facts which the more practical traveller requires," and he has succeeded in writing a delightful, chatty account of a county in which Londoners have an especial interest. The history, architecture and folk-lore, the animal and plant life of the county, and the customs and characteristics of the people are all noticed by Mr. Lucas and skilfully woven into a pleasing narrative. The illustrations, of which there are nearly eighty, are excellent, and add greatly to the charm of the book.

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LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

Learned Societies.

THERE are two other defects of the present system of reporting on papers to which I desire to direct attention. In the first place there are certain mathematicians who resemble the Athenians in the time of St. Paul, who "spent their time in nothing else, but either to tell, or to hear some new thing." They are consequently averse to reporting in favour of a paper unless it contains new results. Against this excessive craving after novelty I emphatically protest. Many interesting results frequently drop out incidentally in the course of a long and complicated investigation, whilst others have been originally obtained by some cumbersome and antiquated process,¹ and in my judgment a paper which supplies concise, simplified and improved demonstrations of results of this character is quite as valuable as one which is devoted to the investigation of new results.

In the next place, as a general rule, none of the councillors present have read the paper unless any of them happen to be referees. Moreover, a good many of the councillors present, even if they had tried to understand the paper, would be quite incapable of expressing an opinion as to its merits, and I well recollect that I myself have sometimes experienced considerable embarrassment when invited to vote officially as a councillor against the publication of a paper which lay outside my own line of reading, and I have sometimes got over the difficulty by abstaining from voting.²

I regard Prof. Bryan's suggestions as altogether impracticable. In the first place no person possessing ordinary common sense would run the risk of adverse criticism by consenting to report on a paper relating to a subject with which he was only slightly acquainted. In the next place no author, except a very junior one, would consent to subject his papers to the extensive revision, which Prof. Bryan appears to contemplate, at the suggestion of an *unknown* and possibly a very junior referee. He would probably regard such suggestions as a piece of impertinence (and I recollect one such case in connection with a foreign mathematician), and he would make short work of them by insisting on the society printing his paper as it stands or returning the manuscript for publication elsewhere.

I believe that every Royal Academician possesses the privilege of hanging a certain number of his pictures every year, and I see no reason why a similar privilege should not be extended to members of learned societies with regard to the publication of their papers. A. B. BASSET.

Fledborough Hall, April 16.

Department of International Research in Terrestrial Magnetism of the Carnegie Institution.

THE trustees of the Carnegie Institution at their annual meeting last December authorised the establishment of what is to be known as the "Department of International Research in Terrestrial Magnetism." An allotment of twenty thousand dollars was made with the expectation that if the proposed work should be successfully organised, a similar sum would be granted annually for the period requisite to carry out the plan submitted by the writer, as endorsed by leading investigators, and published in "Year-book" No. 2 of the Carnegie Institution.

The undersigned has been appointed director of the department, and has been given full authority to organise it, beginning with April 1. Arrangements have also been made so that the magnetic survey and magnetic observatories of the United States, conducted under the Coast and Geodetic Survey, will remain in his charge as heretofore.

¹ The method by which Euler's equations for the rotation of a rigid body used to be proved is an example.

² A very glaring example of the imperfections of the present system will be found in the *Phil. Trans.*, A. 1892, in connection with Mr. J. J. Waterson's paper.

The general aim of the work is "to investigate such problems of world-wide interest as relate to the magnetic and electric condition of the earth and its atmosphere, not specifically the subject of inquiry of any one country, but of international concern and benefit." The prime purpose, therefore, of this department is not to *supplant* any existing organisation, but rather to *supplement*, in the most effective manner possible, the work now being done, and to enter only upon such investigations as lie beyond the powers and scope of the countries and persons actively interested in terrestrial magnetism and atmospheric electricity.

At first principal stress will be laid upon the complete reduction, discussion and correlation of the existing observational data, and upon early publication of the results in suitable form, in order to exhibit the present state of our knowledge. In this way will be revealed the gaps to be filled, and the direction of future and supplementary investigations will be suggested. While, however, this will constitute at first the chief work of the department, it is likewise proposed to embrace favourable opportunities for supplementing, by observation, the existing data and to co-operate with others in the observing of such of the earth's magnetic and electric phenomena as are of momentary occurrence, and the investigation of which is of great importance.

Details as to the method of work to be followed by the department and the investigations undertaken will be made known later. It is proposed that, whenever feasible, those having certain pieces of work already in hand shall be invited to associate themselves with the department.

A word of explanation as to the "international" character of the undertaking. As all the funds are supplied by the Carnegie Institution, it will not be possible to organise this department in accordance with the customs governing organisations the funds of which are contributed by various nations in concert, such as, for example, the International Geodetic Association, the "International Catalogue of Scientific Literature," &c. While, however, the basis of organisation cannot be "international" in the sense usually defined by such bodies, it is the intention to conduct the work with the counsel of an advisory board composed of representative persons, irrespective of country. The work itself, as already stated, is not confined to any one country, and in this sense the department is to be truly "international."

That an important step has been taken by the Carnegie Institution will be patent to all who are interested in the development of our knowledge of the earth's magnetism and electricity. Prof. Neumayer, one of the representative investigators who endorsed the undertaking and promised support, expressed himself thus when this project was submitted to the Carnegie Institution:—

"I am of opinion that if this plan reaches its fulfilment it is the most important step ever taken for the development of our knowledge of the earth's magnetism. The thought which underlies it must appeal to everyone who has ever been engaged in geomagnetic investigations. In no other branch of geophysics is it more essential to extend the inquiries over the entire earth. Magnetic research, to be successful, requires the cooperation of the most competent investigators of all countries."

All communications intended for the department should be addressed as below.

L. A. BAUER.

Department of Terrestrial Magnetism, Carnegie Institution, Washington, D.C.

The Formation of Coral Reefs.

IN NATURE for February 18 there is a paper by Mr. J. S. Gardiner giving a concise account of his theory of coral reefs. A more extended paper by him on the same subject appeared in the *Geographical Journal* for 1902, and was followed last year by one by Mr. Günther on erosion on the west coast of Italy; the latter author proves, on a coast with a very strong sand scour and with loose calcareous rocks on the fore-shore, that below "wind and water" mark no erosion to any appreciable extent goes on. Mr. Günther's observations coincide with those of every day experience, and should no one have already protested against Mr. Gardiner's views, will you let me record my most

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vigorous dissent? The principles of geology are so little understood, and it requires such a level head in sorting out the true from the false from among the apparently conflicting evidences that one has to deal with, that I think when such a glaring misconception of the processes of nature as that of Mr. Gardiner's is repeated in scientific journals some stand should be made before more mischief is done. It is a disadvantage on my part not to have worked on coral reefs myself, but I have been working for ten years on a coast which is fringed with coral reefs only a short way from where I have seen it, and I have therefore been able to study the base of the reefs without the disguising covering of coral limestone. I have seen beaches crowded with the shells of animals still living in the adjoining sea, raised two and three hundred feet above sea-level, and a short distance away I have seen consolidated sand-dunes going far below sea-level; while from the submerged plateau the edge of which is called the Agulhas Bank, Dr. Gilchrist has dredged large water-worn boulders far out to sea. South Africa generally is a rising area, but in a sinking area the exact converse is true, namely, that with a general sinking local elevations must occur. This last is not a new statement of fact, but a well established experience, and one that has been treated of again and again, for instance, by Suess, in his "Antlitz der Erde," vol. ii. chapter i., and is one which I believe can be proved on any coast—it is well brought out in Mr. Günther's paper, and in many descriptions of coral islands; with such a statement before one, I am at a loss to understand where the room comes in for Mr. Gardiner's theory, or where are the difficulties which led to the manufacture of the hypotheses of Messrs. Murray and Agassiz. The one fundamental idea that dominates the whole conception of the earth's structure is that the crust is never at rest, but is incessantly rising and falling; and a corollary is that each great rise or fall is never continuous, but is the result of the surplus of a series of + and — movements. If we are to adopt Mr. Gardiner's view that submarine erosion can cut down solid rock to 200 fathoms below the surface of the water, geology must be deposed from its pedestal as a science, and relegated to the class which includes gnostic theology and such like.

ERNEST H. L. SCHWARZ.

South African Museum, Cape Town, March 9.

MR. SCHWARZ, as far as I understand him, questions the effects, if any, of submarine erosion and the necessity for any view except that of subsidence to explain the formation of coral reefs. I fail to see the parallel between the west coast of Italy and the Maldives, but it is obvious that wherever submarine currents exist there must be erosion, its extent depending on their force, &c. I would refer Mr. Schwarz to the detailed work on currents and the effects of organisms in my full paper on the Maldivic and Laccadive Archipelagoes in the "Fauna and Geography of the Maldives and Laccadives." He will find there certain references, but in the same connection he should read Prof. Agassiz's numerous papers issued from the Museum of Comparative Zoology at Harvard, and Prof. Max Weber's and Captain Tydeman's reports on the Siboga Expedition.

In common with most recent workers on the subject I have treated of my difficulties in respect to the acceptance of the subsidence theory in the paper already cited and in my report on Fiji, and Mr. Schwarz will pardon me if I do not feel disposed to re-state them. The gentlemen he refers to have probably stated theirs. We are doubtless well aware of the general facts mentioned by Mr. Schwarz, and I feel sure that we shall be only too happy if he will explain our difficulties and assist us in arriving at the truth on this important question.

I may take this opportunity of pointing out that, according to the report of the Coral Reef Committee, Funafuti has probably been largely formed by subsidence, and hence may be an example of the fourth mode of formation mentioned in my article.

J. STANLEY GARDINER.

Zoological Laboratory, Cambridge, March 31.

Demonstration of Magnetostriction.

IN A recent number of NATURE (March 24) Prof. H. Nagaoka describes a method for demonstrating the change of length of an iron wire by magnetisation. I have for